THE

PSYCHOLOGICAL BULLETIN

THE NEED OF AN EXPERIMENTAL STATION FOR THE STUDY OF CERTAIN PROBLEMS IN ANIMAL BEHAVIOR.[1]

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Two great classes of investigations in comparative psychology await an experimental station for their complete solution.

Class I.   Investigations requiring continuity.   (*A*) Those types of investigations requiring the observation of many generations of a given species.   (*B*) Those requiring the continuous observation of an animal from birth to old age.

Class II.   Investigations requiring a larger environment than that afforded in universities located in cites.

Let us consider a few specific, typical problems in each class.

DEVELOPMENTAL PHENOMENA WITHIN A PARTICULAR SPECIES.

Thorndike in his book on ' Educational Psychology ' stated a problem which many of us interested in both human and animal psychology would like to see settled.   It concerns the old, vexed question as to whether a particular type of behavior, acquired during the lifetime of an individual of a given species, will be present in the offspring of that individual.

Thorndike's words are as follows (p. 63) : " The obvious way to settle our question [as to whether mental traits are inherited] is not by contemplating these inferences from present knowledge of the process of development, but rather by making the crucial experiment of letting animals acquire some mental traits and observing the nature of the offspring.   No such experiments of a decisive nature have been made.   If for generation after generation mice were offered palatable

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[1] This number, dealing especially with topics in comparative psychology, has been prepared under the editorial care of Dr. John B. Watson.

food always in the shape of yellow cubes smelling of grease, and unpalatable food always in the shape of white balls smelling of cheese, were kept in a cage so arranged that on going into a certain alley they always received an electric shock, and were otherwise given a chance to learn certain habits, an observer could measure for generation after generation the quickness of formation of these habits and detect the slightest improvement. Ten or twenty generations would thus give a final answer to an ancient quarrel."

While the difficulties in working out this problem would be many, they are not insuperable. In the white rat we have an alert, intelligent and active animal — an animal which exists only in captivity, in consequence of which experimental conditions impose little hardship upon it. The animal breeds all the year round and its period of gestation is only 21 days. The white rat begins to acquire habits early and under experimental conditions can master ten to fifteen ' problems ' before it is 30 days of age. Again, the white rat is sexually mature at 60–70 days. This makes it possible for us to have a new generation every 80–90 days.[1] In five years, barring accidents, we should have an unbroken succession of 20 generations of a rat family.

We would suggest that this problem could be attacked *at an experimental station* in a little more elaborate way than that suggested above by Thorndike. Let us suppose, first, that we have at our experimental station a series of twenty-five standard problems, varying in complexity; secondly, that we have an unvarying routine in teaching the first animals these problems; thirdly, that we keep a complete set of records of the time, errors, etc., made in learning these problems. Let us start our experiment by presenting, in a fixed order, these twenty-five problems to ten rats, five males and five females—all from different families. These ten rats could master these problems by the time they were seventy days of age. They would then be allowed to breed. A selection of the young would follow. Those selected would be taught the problems under the same iron-clad routine that their parents had to follow — the number of young selected would be determined by the natural limits of the investigator's time. This same routine could be followed, year after year, until the behavior record of ' Smith White,' the twentieth, showed a marked superiority over that of ' Smith

[1] It might be objected here that the animals at the age of 70 days are too young for the acquired type of behavior to have had its full effect upon the germinal cells (?) of the organism. This objection could be met by restraining the animals from breeding until they were, say, 140–150 days of age. This would have the effect of approximately doubling the time necessary for the investigation.

White,' the first, or until the investigator might be convinced that tuition from generation to generation has no effect.

Along with this broader and more detailed treatment of the problem, some such minor investigation as is suggested by Thorndike with reference to the food of the rats, might be carried out.

In conducting this investigation as we have just indicated we would in the end have data upon both males and females in learning those standard problems. Differences between the sexes in their ability to inherit ' mental traits' would thus be detected if such exist.

Developmental Phenomena in the Life of an Individual.

Only those who have attempted to study seriously some phase of mammalian behavior can appreciate the enormous amount of time it requires. The reactions are so complex and the results have to be presented in such large numbers that one burdened with instructorial work hesitates to start a new problem. Then, too, there is no doubt in our mind that the published results of behavior tests would be more complimentary to the animals were we in a position to devote more continuous time to them and to watch and to control their early development.

A glance at the bibliography of comparative psychology for the past two years will show that we have ten studies on the behavior of lower organisms to one on mammals. The reason is not far to seek if we consider the comparatively small amount of care and expense the lower organisms require. A jar of water and a wisp of hay suffice to give paramecium a suitable habitation. But a mammal, even so low in the scale as the rat, must have intelligent care.

But let us mention some of the specific types of investigations to be considered under Class I., *B.*

The effect of continued tuition upon the behavior of the individual of a given species is a general problem which must be solved before we are upon firm ground in our interpretations of specific results. Let us consider the rat again. Suppose that in this connection instead of 25 standard problems we have 100 problems increasing in complexity from 1 to 100. Let us again keep an accurate record of the time, errors, etc., of the learning of these 100 problems. Ten rats would be a sufficient number to start with. It would probably take these ten rats 18 months to learn all 100 of these problems. At the end of that time our records of these ' educated rats' would show separately, for each of the 100 problems, the average time of the first successful trial and the difference in time between this first successful

trial and the succeeding successful trials up to the point where no further improvement could be detected.

Now, in order to obtain data for drawing inferences as to the effect of tuition, let us put ten ' uneducated' rats to work upon, say, problem 80, ten other uneducated rats upon problem 85, ten upon problem 90, ten upon problem 95, and finally ten upon problem 100. One cannot predict absolutely that the records in the two cases would be different, but we should confidently expect such to be the case. In our own experience with the behavior of rats we have the feeling that they work more quickly and more intelligently upon complex problems if they have had experience with simpler problems. This may not be true, but we should like to see it tested upon several species of mammals.

In working out this problem the records of the males would again be kept separate from those of the females.

' Imitation ' is another type of behavior which is of general interest to the student of psychology. The study of this mode of reaction requires an enormous amount of time if we desire to test it to any extent under conditions of control. We have shown elsewhere that the only cases of it on record are the apparently sporadic cases cited by Kinnaman, Hobhouse and Porter (neglecting the song imitation in the birds of Conradi, Scott and others). We would venture to suggest that the backward state of knowledge which this phase of animal behavior is in, is due in part at least to the backward state of two questions more elementary than that of imitation.

The first and fundamental difficulty in the way of a study of imitation lies in the *roving* character of *animal attention*. Hobhouse mentions this difficulty with his dog and cat. It is familiar to all. A dog may apparently be *looking* at *you* intently when all the time he is interested in the odor of your boots, as proved by the fact that when you get to the most interesting point of your discourse with him he drops his eyes, walks up to the boot, and investigates it. But that he can be trained to attend to your actions may be shown by any dog which can hunt both rabbits and squirrels. The moment you take your gun he bounds for the woods. Ordinarily he has hunted rabbits more often than squirrels. So he starts off immediately with eyes and nose upon the ground. You stop him, take him to a tree and pat it and say ' squirrels,' repeat it two or three times, and, if he is a well-trained dog, he will hunt squirrels for the rest of the day. We ought to know and must know in some more exact way to what extent we can train an animal to the habit of giving attention to what the experimenter does.

Intelligent dogs with acute visual powers, we should suppose, would serve this purpose best. Infinite time and infinite patience with the education of young animals alone will settle the question as to how far the attention may be cultivated. The exhibitions of trained dogs and horses — usually given by charlatans, since they alone have time to live alongside of their animals — would seem to show that the attentive processes may be carried so far in these animals that they can observe minutiæ which escape the eye of the ordinary human observer.

On account of the limited time at our disposal, those of us who have experimented upon animals heretofore have had to study dogs, cats, monkeys, etc., which have not been generally educated nor have they had the specific habit of *giving* attention cultivated. Such a method is somewhat analogous to presenting calculus to a South Sea Islander. It does not appeal to his practical needs, and is not in line with his previous education. Consequently he can make no progress in it. The result might be far different if we took a baby Islander and led him to calculus through a long series of gradual steps.

Another difficulty which is in the way of a thorough study of imitation lies in the fact that we are hopelessly ignorant, in any exact way, of the perceptual processes in animals. Through what avenue or avenues of sense does our animal get his impressions? Certainly we must know this before we can hope to attract the attention of our animal. If a rat uses vision only for crude general orientation, what is the use of letting him *see* another rat tearing away the papers from the hidden door of a problem box and then expecting him to imitate the behavior? It would be foolish to give the dog mentioned by Romanes — the one who always followed the peculiar-smelling hunting boots, no matter who wore them — the visual perception of the removal of a secret bolt and then expect him to go and immediately withdraw the bolt.

If these difficulties are agreed to as being real, they would seem to account in part for the negative results of Thorndike's experiments on imitation in dogs, cats and monkeys.

ENVIRONMENT AND DEVELOPMENT.

Few psychological laboratories in the United States have adequate space for the care of mammalia. None have adequate provision for birds and insects. Harvard's new philosophical building forms a partial exception. At Chicago a single large basement room is all that can be spared for comparative purposes. Clark seems to be somewhat better prepared than this. The conditions at Cornell and

Columbia are, I believe, not much better than here. To properly
carry out experiments in mammalian behavior alone, we need condi-
tions suitable for the breeding and growth of a large number of animals.
Such conditions are at hand only when we have a large space suitably
divided up.   Outside of the lack of space, the universities located in
cities have another difficulty in the way of housing any considerable
number of dogs and cats under a single roof.   They are likely to find
themselves under arrest for keeping a nuisance.   In a noisy community
the animals are practically never in a state of repose.

In addition to the need of a large, well-lighted and well-warmed
animal house, the need of a considerable stretch of ground at once
makes itself felt.   This is clearly manifest when we mention the study
of the homing instincts in animals.   The need of the study of this
problem is great.   A perusal of the literature on the subject — and it
is vast, especially the French literature — will show that even the
facts about the 'return' are in a chaotic state.   The actual per-
ceptual (?) factors involved in the return are in a still more hopeless
state.   Hypotheses are rampant.   They range all the way from the
one which endows the pigeon with the ability ' to directly perceive
the end ' — even the curvature of the earth forming no barrier to this,
in view of the fact that the pigeon may use the infra-luminous rays
(Hatchet-Souplet ; Duchâtel) — to that of Cyon who believes the
pigeons have a ' special nasal sense ' distinct from ordinary olfaction
which permits them to distinguish the currents of air suitable to
return in to the nest.   Still another — that of Reynard — claims that
the method of return follows the law of Contre-pied — *i. e.*, the
pigeon has some kind of registration apparatus which marks down all
the turns and twists of the road over which he is borne away from the
nest.   This registration apparatus works even if the bird is carried
away blind-folded.   When the bird is released the registrations some-
how become effective and when the bird has ' unwound' himself he is
once more at home !   The moment he gets in the familiar neighbor-
hood of his nest he at once begins to use his vision.   The law of
Contre-pied would of course mean that the return would be circuitous,
but as soon as the bird comes upon the familiar landmarks he takes
the shortest pathway home.

Many of these hypotheses are mutually exclusive.   We ought not
to be content until we can at least get a theory which will unify all the
facts — but the facts themselves must come first.   The obtaining of
the facts is a delicate problem.   The French investigators are agreed
(some of them at least) that a pigeon can return to the nest from a dis-

tance of 500–600 km. over an ocean pathway which, for our senses at least, can offer no kinds of distinguishing marks.

The factors active in the homeward flight of the bee are just as unknown (or rather just as disputed). Bethe, after many experiments on the influence of audition, olfaction, vision, memory images and magnetic force on the return of the bee, finally concludes in the following words: " Die Bienen folgen einer Kraft, welche uns ganz unbekannt ist, und welche sie zwingt, an die Stelle im Raum zurück-zukehren, von der sie fortgeflogen sind. Diese Stelle im Raum ist gewöhnlich der Bienenstock, sie muss es aber nicht nothwendiger Weise sein. Die Wirksamkeit dieser Kraft erstreckt sich nur auf ein Gebiet von wenigen Kilometern im Umkreis." Surely we are not content to let the matter rest here. Fabre finds this homing instinct in the male cat — the factors involved in his return though are just as unknown as in the above-cited cases. This same orienting instinct is found by the Peckhams to be present in the wasps.

If distant orientation does involve some ' sense' or ' force' which is foreign to our own mental makeup, we ought at least, in the words of James, to get as much ' knowledge about' it as we can.

It is fully apparent, we hope, that the behavior of insects and birds must remain in a backward state until an experimental station shall have been established, which will give to the animals naturalness in environment and to the investigator ample space for experimentation.

In thus emphasizing the need of an experimental station for working out the larger problems in comparative psychology it is not our purpose to discourage or disparage the value of the work of the investigator who must necessarily labor under time, space and financial limitations. Some of the best results we have to-day are monuments to what can be done under such limitations. Our purpose has been rather to show that there were broad fundamental problems which will soon have to be solved if our interpretation of individual results is to be accurate. Such an experimental station will not remove the need of as many individual investigators as may be attracted to the field. It will aim, rather, on the one hand, to make possible a more complete interpretation of their results, and, on the other hand, to suggest new problems which can be individually carried out. It will stand for centralization, coördination.

It is obvious that even under adverse circumstances comparative psychology has completely justified its existence. But now that the ground is broken and the more obvious and surface problems have

been successfully attacked, we stand in great danger of allowing our interest to flag — especially is this true when we view the difficulties of making our studies of mammalian behavior more exact and more intensive.

In conclusion we may say that the need to the psychologist of an experimental station for the study of the evolution of the mind is as great as is the need to the biologist of an experimental station for the study of the evolution of the body and its functions.

ADDENDUM TO DR. WATSON'S PAPER.

It may serve to enforce both Doctor Watson's last remark and also the general contention of his paper to note that the recommendations made to the Carnegie Institution by the original 'Advisory Committee on Psychology' in 1902 (see the year-book of the Carnegie Institute, No. 1, p. 197) placed *first* the establishment of a station for animal psychology in coöperation with zoölogy; and the reasons there given are similar to those of Doctor Watson. Furthermore, the recommendation was reached in consultation with the Committee on Zoölogy. That report also included a consensus of opinions from certain of our leading psychologists.

J. MARK BALDWIN.

PSYCHOLOGICAL LITERATURE.

COMPARATIVE PSYCHOLOGY.

La psychologie comparée est-elle légitime? ED. CLAPARÈDE. Arch.
de Psychol., 1905, V., 13-35.

In view of the fact that certain biologists have recently denied the
justification of the study of comparative psychology on the ground that
we are ignorant, and will always remain ignorant of the fact as to
whether animals are conscious or not, Claparède proposes to examine
critically the arguments of those who would thus suppress it.

One of the first undertakings of such biologists has been to devise
a new and 'objective' nomenclature in which to describe the reactions
of animals. As is well known, Beer, Bethe, von Uexküll, *et al.*,
proposed this nomenclature some years ago. For them the word
'sensation' has been replaced by *reception*, 'sense organ' by *reception
organ*, 'memory' by *rémanence de l'excitant*. Visual, auditory,
tactual and gustatory sensations are called by their objective equiva-
lents, photo-, phono-, tango-, and chemo-receptions. From time to
time others have added to this new nomenclature. M. Nuel (*La
Vision*, Paris, Doin, 1904) is the latest author to become listed in the
ranks of those arrayed against comparative psychology. Indeed Nuel
goes so far as to attack the validity of the psychological method in
general. He tells us that it is not only the vision of animals but also
that of man which he has taken upon himself to explain 'without the
help of any psychological notion.' Claparède remarks that this attempt
is entirely new and that M. Nuel has a perfect right to claim for it
'an incontestible right of priority.' 'The most radical authors,' says
Nuel, 'believe that they are obliged to apply their principles only to
the vision of animals.' Nuel is further convinced that 'visual phe-
nomena demand not a psychological but a physiological explanation.'
('As if these two orders of explanation were contradictory!'
Claparède justly exclaims.)

Claparède then goes on to show the reasons for this dissatisfaction
on the part of these biologists with the psychological nomenclature.
They arise from the gross anthropomorphic descriptions of animal
behavior given by Romanes, Lubbock and others. Our author very

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justly asks the biologists what the fault is in these explanations of
Romanes and others: "Is it because their language is psychological?
By no means. The explanation, by 'desire,' by 'curiosity' (referring
to Romanes' description of the reaction of the moth to the flame), is at
fault not because it is *psychological* but by reason of the fact that it
does not conform to the principle of [Lloyd] Morgan; it is not the
simplest explanation possible."

Claparède then, by a series of arguments which are at times almost
vitriolic, shows that this new objective terminology is often times inex-
act, inconvenient and obscure, and at all times pedantic.

But the gravest danger to comparative psychology, according to
our author, lies not in the mere description of animal reactions in
'objective' terminology — this is a merely passive evil. There is a
more serious and active danger. "Wishing to account for their ob-
servations in comparative biology in physiological language, these
investigations of the new school find it necessary, in order to fulfill
the obligations which they have taken upon themselves by reason of
their so-called objective nomenclature, to lower the facts to the level
of their vocabulary. They are like the children's tailor who, being
obliged to dress an adult in one of his costumes, cuts off the arms and
legs of his unhappy customer! — and they are forced to consider as
very simple reflexes phenomena which are without any doubt the
result of a complex cerebral activity, but an activity, it is true, which
we cannot designate otherwise than by the states of consciousness
which are the subjective concomitants of it." It is in this connection
that we find the names of Bethe, Loeb and Nuel again mentioned.

The constructive side of the paper is concerned with showing
that the suppression of comparative psychology leads logically to the
suppression of genetic psychology and finally to that of adult human
psychology.

While we sympathize with Professor Claparède in his endeavors
to have psychological phenomena expressed in psychological termi-
nology, still we feel sure that his fears for the future of comparative
psychology are unfounded. In the first place the *psychologists* will
see to it that this science is not suppressed. In the second place we
hasten to assure M. Claparède that whatever may be the conditions on
the Continent, there is no tendency on the part of American biologists
to suppress it. On the contrary, all of our biological laboratories are
encouraging such studies. Who could want a more sympathizing
attitude than that shown by Donaldson, Howell, Minot, Whitman,
Wesley Mills, Parker, Wheeler, Jennings, Davenport, Hodge, C. J.

Herrick and the late C. L. Herrick? The only possible exclusions from this list are Loeb and those trained under his influence. The value of Loeb's contributions to comparative psychology must not be underrated even if later investigations have thrown certain of his results into disfavor. In addition to the intrinsic value of his work we can trace back many contributory investigations in this field either directly or indirectly to his stimulating influence.

There is and can be then no 'war to the death' in this country between comparative psychology and biology. Comparative psychology is rather the common meeting ground of the psychologist and the biologist. It is having and will continue increasingly to have a a marked influence upon the training of both the psychologist and the biologist. The psychologist without biological training, who is busied with the study of animal behavior, finds little to express; the biologist, who has no psychological training but is engaged upon the same task, cannot express what he finds — or else, if he tries to express it, he runs either into the absurdity of forming an 'objective' nomenclature or he becomes inexact in his use of psychological terminology.

Ein Fall von Ueberlegung beim Hund? WILHELM AMENT. Arch. f. d. ges. Psychol., 1905, VI., 249–253.

In this article an anecdote concerning the behavior of a dog is reported. The dog under consideration (a two-year-old 'Zwergpinscher') was accustomed to sit on a chair in front of a window which overlooked the neighboring houses and yards. One cold day the dog found his window so thickly coated with frost that his customary view was interfered with. "Was tut nun der Hund? Er leckt mit seiner Zunge die Eisblumen einfach weg, bis das Fenster wieder durchsichtig geworden ist." As soon as a round area about the size of a plate had been cleared away with difficulty, the dog stopped cleaning the window and took up the more natural canine occupation of watching the cats in the adjoining yard. The dog repeated this behavior several times during the winter.

The author tells us that this interesting bit of natural history can be interpreted in a number of different ways. His interpretation is given in the following manner (a rather free translation, by the reviewer, of the author's own dignified and somewhat involved German): By means of the experience of wiping with his snout, the dog hits upon the licking away of, first, the softened layers of ice, and later the more solidly frozen ones. That the dog straightway hits upon the

method of licking is not so surprising, when one thinks of how often during the day the dog licks himself, everything and everybody, and that he surely knows that by licking he can remove a disturbing object. All things considered, we seem to have here the joining together of a series of experiences, ideas (Vorstellungen), partly different from one another (wiping with his snout, licking with his tongue), partly analogous (*i. e.*, the licking away of other things and the licking away of the frost on the window), in the interest of an end (*i. e.*, looking out).

The author expresses the series of events otherwise in the following words: " Auf Grund der allgemeinen Erfahrung, dass man mit der Zunge Gegenstände weglecken kann, und der besonderen, dass sich am Fenster die angelaufene bzw. angefrorene Masse mit der Schnauze wegwischen lässt, gelangt der Hund zur Ueberlegung, dass er die Masse mit der Zunge wegwischt bzw. wegleckt." This is the characteristic of a conclusion (Ueberlegung) as illustrated in the practical procedure of the common man. If this act of the dog were done by a human being we should not hesitate to call it Ueberlegung — it is only when the act is done by an animal that we begin to bristle at this designation. I may remark, finally, that I wish to reserve judgment upon the degree of voluntariness of the mental processes of the dog in performing this act: I have used the term ' Ueberlegung' always and not ' Schluss'; not because I do not hold that this ' Ueberlegung' is equivalent to an actual psychical ' Schluss,' but because there is to-day an irritating war of words, between the logicians and the psychologists, over the concept of ' Schluss.' The author modestly concludes by saying that if anybody else can offer a simpler explanation of the factors involved in this act, he has his, the author's, permission.

The reviewer thinks it is safe to say that upon the basis of the anecdote as it is reported, no one would try to frame an hypothesis as to what mental factors are involved in this act of the dog. Most of us who really have the interest of animal psychology at heart would have modified and controlled the situation by a series of simple and obvious experiments.

If the article under review had been appended to the Archiv as a *note* (and put in some inconspicuous place) its publication would have had a doubtful justification: that it should occupy the first five pages of an important psychological journal is inexcusable.

                                                              J. B. W.

Respiration and Emotion in Pigeons. John E. Rouse. Jour. of
Comp. Neurol. & Psychol., 1905, XV., 494–513.

The chief value of the experiments under review is in the contri-
bution attempted towards methods of control. Respiratory change is
used as the index of psychical status, since this shows minute varia-
tions in stimuli and is easily recorded by the pneumograph. The
support for the birds which were being experimented upon was a
level horizontal board, in which an oval opening had been cut just
large enough to admit the breast of an adult bird. In this opening the
birds were secured in such manner that the breast projected through.
A breast-plate with tambour connections was adjusted from below,
thus permitting the curve of the sterno-vertebral breathing movements
to be secured. The feet were held in place by tape fastened to hooks
behind. A box covered the bird and the experiments were made with
darkened room. An electric signal marker was used, which, writing
beneath the respiratory line, automatically indicated the time of giving
the stimuli. In the case of the visual tests this was rendered possible
by using a double-contact key. A third pointer, connected with a
metronome beating half seconds, showed the rate of the drum move-
ment. The metronome was enclosed in a felt-lined box. The
stimuli were pistol shots, bell ringing; odors of turpentine, ammonia,
asafœtida, oil of bergamot, and lily of the valley; red, yellow, green
and blue lights; lights of the same color, but of different intensities;
concussions upon the table holding the support, and upon various
parts of the room. Color preference was tested independently, by
illuminating one half of the box containing the bird, with light of one
color and the other half, simultaneously, with light of a different
color. The lights were furnished by incandescent lamps, the glasses
of which were differently colored. The four colors of the visual tests
were employed in this experiment also. Each color was used succes-
sively with the three others and in every case the position of the bird
with reference to the two lights was noted.

The principal results of the series of experiments are as follows:
(1) All of the stimuli produced quickening in respiration and also
changes in amplitude and contour of the curve. These changes were
strongest in the case of concussion and sounds. (2) Those sounds
which might be thought of as meaningless to the birds, such as pistol
shots, very early lost their stimulating power, while much weaker but
significant sounds, such as bird noises, retained their power. (3) No
direct relation between intensity of light and the amount of reaction
was perceived. (4) Agreement was shown between color preference

and the respiratory quickening caused by colored light. This was taken to prove concomitance between quickening of breathing and agreeable feeling.

The mass of entirely conflicting evidence hitherto discovered, which relates to the last conclusion, however, continues to stand and to negate the validity of the conclusion which the author rather emphatically gives it. The conclusion is rendered still more unwarrantable by the evident fact of the failure in the experiments herewith reported to exclude external and manifestly disturbing stimuli; and by the further fact that the number of records upon which the conclusion is based are comparatively few.   A. A. FARLEY.

UNIVERSITY OF CHICAGO.

Die Orientierung der Brieftauben. G. H. SCHNEIDER. Zeitsch.
f. Psychol. u. Phy. d. Sinnes., 1905, XL., 252-279.

In a note appended by the editor of the Zeitschrift to the above article, it is stated that Dr. Schneider had intended using the results there communicated in a second edition of *Der Thierische Wille.* Owing to the death of Dr. Schneider a second edition of this book was apparently never completed. It is impossible to make out from the article itself just when Dr. Schneider completed this work. He apparently began it in 1886. The author gives only five references to the literature — the most recent of the five bearing the date 1878. No mention is made in the paper either by the author or by the editor of the rapidly increasing modern literature on this subject.

The experiments are concerned mainly with the education of young carrier pigeons and with the short flights of adults. The question he sets out to prove is this: are the pigeons guided by an inborn sense of direction, which is unknown to us ? Or are they guided by the eye? And if the latter supposition is assumed to be true, how are they influenced by topographical relations?

Dr. Schneider's method of experimentation was essentially like that adopted by all other investigators of the pigeon's behavior in distant orientation. The birds were transported in a basket by carriage or rail to the desired distance and were there released one by one. The time of the release, state of the weather, number and characteristic markings of the bird, were all carefully recorded. Trustworthy boys stationed at the côtes recorded the time of the return.

The experiments were made chiefly in and around Pössneck. The distances used were very short — from 3 km. to 42 km. Certain experiments were made to determine the relative ease of orientation between releases made in valleys and on mountains.

His conclusions are numerous, the one of chief interest being as follows: " The assumption that the carrier pigeons possess an inborn sense of direction is an error; for if this assumption were true, then the young pigeons ought to find their way equally well. The investigations have shown, especially those at Könitz, that young pigeons, even at relatively small distances from their home, have the greatest difficulty in finding their way back when the vicinity is at all strange to them, and their home cannot be directly seen." He then concludes that the young birds utilize, in their early flights, the familiar groups of houses, mountains, etc., and that the distances to which a bird may be taken and return may be increased commensurately with the increase in the development of his 'topographical memory.' The author believes that the pigeon can develop not only ' Erinnerungsbilder ' but even ' Gedächtnisse.'

Dr. Schneider does not discuss the more difficult feats of the carrier pigeon. He says nothing of their long flights over the ocean. He says nothing of the so-called ' voyaging' pigeons of France. These birds travel over the continent in wagons. A stay of one or two hours in a town enables these pigeons to return to it. He has missed the point in the arguments of those who hold that there are factors in distant orientation which are not explicable by visual sensation or even visual memories. In the first place nobody, we believe, would deny that the pigeon uses vision where he can. And again, it is a mistaken use of the term ' sense of direction' to assume that it does not have to develop. Consequently we should not expect the young birds to return as well as the adult. The term ' sense of direction' is used by careful writers with the implication that there is a definite psychophysical possibility of its being developed — just as there is a definite psychophysical possibility of visual sensations being developed.

In conclusion, we may say that if the article under consideration was published out of respect to the memory of Dr. Schneider, the space allotted to it in the Zeitschrift was well used. But if the editor of that journal thought the article would contribute to our knowledge of the factors in distant orientation, or that it was even a good résumé of the facts already obtained, he was laboring under a misapprehension. The article should not have been published under any circumstances without someone's bringing the literature up to date. Had this been done it could then have been shown that Dr. Schneider independently reached certain conclusions which would have been of value if they had been made public at the time of their discovery.

Song and Call Notes of English Sparrows when Reared by Canaries. EDWARD CONRADI. Amer. Jr. of Psychol., 1905, XVI., 190–198.

Conradi in this paper tells of some very interesting experiments upon two English sparrows which were captured in a wild state at a very early age.

The first sparrow, captured when one day old, was reared by a canary foster-mother (they make very poor ones according to the author). During the growing period this sparrow was isolated from all other sparrows and placed in a room containing about twenty canaries of all ages. The characteristic sparrow chirp first developed. This was given less and less, being gradually replaced by a 'peep' which is characteristic of young canaries. This sparrow improved in his vocal efforts, gaining confidence finally to chime in when the canaries would burst into song. His own song apparently resembled the confusion of notes which occurred when the three adult canary songsters were singing their best.

The second sparrow was captured when two weeks old. This bird was fed by hand, and was reared likewise in the room with the canaries. The sparrow peep had of course already appeared when the bird was captured. After being with the canaries some time this sparrow developed a song which more or less closely resembled that of the canaries. "At first his voice was not beautiful; it was hoarse. It sounded somewhat like the voice of female canaries when they try to sing. * * * He sang on a lower scale; he often tried to reach higher notes but did not succeed." Later he learned to trill in a soft musical manner. (Certainly something very foreign to a wild sparrow!) The call notes of the canaries were likewise adopted.

These two sparrows were finally segregated from the canaries and placed in a room where they were flooded with notes from the wild sparrows. For the first two or three weeks the integrity of the canary-sparrow song was maintained. At the end of the sixth week, however, they had not only lost the effects of their early training but had adopted the vocal characteristics of the wild sparrows.

On placing the sparrows again under the tutelage of the canaries it was found that they quickly regained what they had lost.

Dr. Conradi tells us that he is to continue this highly interesting and instructive series of experiments upon sparrows actually hatched and reared by canaries.

The Sense of Hearing in Frogs. ROBERT M. YERKES. Jr. of Comp. Neurol. and Psychol., 1905, XV., 279–304.

Yerkes has shown elsewhere that the frog makes no direct motor response to auditory stimulation. In the present paper he investigates more fully than in his previous paper the effect of an auditory stimulus upon the magnitude of the reaction to a cutaneous stimulation when the auditory stimulus is given (*a*) simultaneously with the cutaneous and (*b*) at definite intervals before the cutaneous stimulation.

After an enormous number of careful tests Yerkes summarizes his results in the following words: " The sound of an electric bell occurring simultaneously with a tactual stimulus markedly increases (reinforces) the leg reflex of green, leopard and bull frogs. If the sound precedes the touch by 1″ it is without effect on the reaction; if the interval is not longer than .35″ it usually causes reinforcement, whereas for an interval of from .4″ to .9″ there is partial inhibition of reaction." In a series of curves Yerkes shows the reinforcement or inhibition effect of the auditory stimuli when given at varying intervals before the tactual.

A second series of experiments had for its object the testing of the above-described auditory-tactual reaction when the tympanum of the frog was either partially or totally under water. His results show conclusively that the (green) frog responds to sounds made in the air even when the tympanum is submerged to the depth of 4 cm.

Yerkes also tested the range in pitch to which the ear of the frog is sensitive. He found the range to be from 50 to 10,000 vibrations per second. (Single or double? The Galton and Appunn whistles were used in testing the upper limit; tuning-forks, electric bells with metal and wooden gongs, sudden hammer blows and the human voice were used for the medium and lower limits.)

In order to make sure that the above phenomena were really auditory phenomena and not tactual, Yerkes made some tests upon (*a*) frogs whose tympana had been cut, (*b*) frogs whose tympana and columellæ had been cut, and (*c*) frogs whose auditory nerves had been cut. The results show that sounds still modify reactions of the frog after the tympana and columellæ are cut, while cutting the eighth nerves causes the entire disappearance of the influence of sounds.

In conclusion, we may say that Yerkes' results are definite, clear-cut and valuable.

Bahnung und Hemmung der Reactionen auf tactile Reize durch akustische Reize beim Frosche. ROBERT M. YERKES. Archiv f. d. ges. Physiol., 1905, CVII., 207–337.

The results of the experiments reported in this paper are similar to, and in part identical with, those reported in the paper reviewed immediately above.   As may be seen from the review referred to, Yerkes shows that when certain temporal relations exist between the auditory stimulus and the tactual, the former tends to reinforce the reactions of the frog to the latter; while under certain other temporal relations of the two stimuli, the auditory stimulus tends either to inhibit the tactual reaction or else leave it unchanged.

In the present paper these phenomena are discussed under the terms Hemmung and Bahnung.

We suppose that Dr. Yerkes' reason for making two reports upon very similar, if not identical investigations, was to allow for a separate treatment of the purely physiological aspects of the experiments and the psychophysical.

In the paper under review, the author places this phenomenon of reinforcement and inhibition in the reactions of the frog in its relation to the work previously done upon this subject by Bowditch and Warren (The Knee Jerk and Its Physiological Modifications), A. Cleghorn (The Reinforcement of Voluntary Musculatur Contractions), L. Hofbaur (Interferenz zwischen verschiedenen Impulsen im Centralnervensystem), and others.

In order that our later comments upon this paper may be clear, we shall translate in full the ' Uebersicht ' of Dr. Yerkes:

1. With the greenfrog (Laubfrosch) a sound which in itself calls forth no noticeable contraction of the leg of the animal under investigation, shows a modification of the reaction to another stimulus if it is given in conjunction with this.

2. The momentary sound stimulus produced by the stroke of a hammer increases the amount of reaction to a simultaneous tactual stimulus. This increase or reinforcement of the reaction amounts to 50–100 per cent. of the mean reaction (mittleren Reactionen) to a tactual stimulus alone. If the auditory stimulus is given before the tactual, reinforcement occurs, the amount of which decreases slowly until the interval between the two stimuli amounts to 0.35″; the auditory stimulus has at this point no apparent effect upon the tactual reaction.   By further lengthening the interval, inhibition is produced which continues for the interval between 0.35″ and 0.9″. Reinforcement is greatest when the stimuli are simultaneous; inhibition is at a maximum when the auditory stimulus precedes the tactile by about 0.4″ to 0.6″. If the interval is 0.9″ the first stimulus has no effect upon the result of the second.

3. Reinforcement is greater in males than in females; the inhibition

is greater and more lasting in females. This circumstance shows evidently that while the males are aroused to activity by certain stimulations of sound, the females have their activity suppressed by similar sounds.

4. A continuous auditory stimulus (such as that made by an electric bell) may produce either reinforcement or inhibition, depending upon the temporal relation of the two stimuli, similar to that produced by a momentary sound stimulus. We found the following differences between the effect of the momentary and the continuous auditory stimulation : A maximum of reinforcement is reached when the tactile stimulus is given about 0.25″ after the beginning of the (continuous) auditory stimulus. Reinforcement continues for an interval of 1.2″, *i. e.*, if the electrical bell is sounded continuously, the tactual stimulus is reinforced from simultaneity up to 1.2″. From this point up to 1.8″ inhibition sets in. Both momentary and continuous (auditory) stimulations produce first reinforcement and then later inhibition of the characteristic reaction to a tactile stimulus.

5. The reinforcement-inhibition curves in frogs are similar to the corresponding curves in man.

6. In the case of the different pairs of stimuli whose interference was investigated, reinforcement and inhibition were present. The first stimulus reinforced the reaction to the second, so long as the interval did not rise above 0.4″, while it later inhibited this. Whether the reinforcement-inhibition curve as it was obtained in the experiments described, can be maintained as valid in a similar manner for any pair of stimuli, in whatever relation their reactions stand, remains to be investigated.

These facts are of course very interesting to the physiologists— but they are interesting to psychologists as well. Indeed it is a little hard for us to see any valid reason for the separation of these two papers. In the first place, although the German article is concerned purely with the physiological aspects of the reactions of the frog, we would surely have forgiven Dr. Yerkes for discussing this phase of the investigation in his English paper, ' The Sense of Hearing in Frogs.' The strictly new material in the German article does not cover many pages. By so doing he would have saved American investigators from having to wade through 30 pages of German, only to find that the English article had already put them in possession of most of the experimental data.

Finally, we should like to enter a protest against having our American researches printed in Continental journals and in Continental languages — at least until there has been an entire reconstruction in the mental attitude of the average German and French investigator to American research productions. Many of us have had to dig out patiently the results of German and French investigations, only in the end to find them marred (and in many cases absolutely valueless) by lack of consideration of the results obtained in our American laboratories.

The Habits of Certain Tortoises.   H. H. NEWMAN.   Jr. of Comp.
Neurol. and Psychol., 1906, XVI., 126–152.

This valuable study of the instinctive life of the five principal
forms of tortoises to be found in Lake Maxinkuckee, in northern
Indiana, does not lend itself to a detailed review.   The main features
of the paper, however, can be briefly presented.

Newman shows that the ' interesting,' and apparently more intel-
ligent forms of tortoises, cannot be kept in captivity and introduced
to the various forms of ' problem boxes ' now so common in the studies
of animal behavior.   The only other method left for studying these
animals is the more laborious one of watching them ' in their daily
rounds and occupations.'   If one does this for a long enough period
of time one becomes able, so the writer assures us, ' to diagnose their
dispositions and comparative intelligence.'

Newman shows that each of the five varieties studied has a definite
' species character.'   He shows also that there is not only a species
character but a sex and an individual character as well.

Certain traits and habits, however, are common to all five of the
species under investigation.   These common characteristics of che-
lonian behavior may be summarized as follows:

" 1. The love of warmth and repose seems to be one of the few
dominant factors in tortoise life.   In some cases they seek warmth to
their injury.   On the other hand, lack of heat is more apt to cause
death than any other factor.

" 2. Extreme wariness when basking is noticeable in all species
that habitually bask.

" 3. There is a marked variation in the degree of fierceness or
timidity exhibited by different species.   These characters seem to run
parallel with an aquatic or a terrestrial habitat, aquatic species being
fiercer than those with a tendency toward a terrestrial life.

" 4. Naturally enough, it is possible to domesticate the less fierce
and less sullen species, while captivity inhibits normal activities in the
fiercer and more sullen species."

The reviewer cannot refrain from mentioning one specific obser-
vation made by Newman in the course of this investigation.   It con-
cerns the question of ' distant orientation.'   He mentions the fact that
the females of *Graptemys* at times lay their eggs in soft earth far away
from the body of water in which they live.   " The eggs hatch, as a
rule, late in August or early in September, the young burrowing to the
surface through the sand.   When they emerge they are covered with
sand that adheres for some time.   Their *instinct* [italics mine] directs

them unerringly toward the water and they frequently have to travel
almost incredible distances before reaching the lake or a tributary
stream. On two occasions I have found recently hatched *Graptemys*
at a distance of about a quarter of a mile from the water, traveling
steadily and in an approximately correct direction toward the lake. At
the observed rate of progress they would reach the lake in about two
days." Evidently there is a good opportunity here to investigate the
factors entering into this ' instinct.' These little animals could hardly
be said to have developed a ' topographical memory,' and certainly if
there happened to be a hill in the way — or even a good-sized log —
they could not have ' directly perceived the end.' What is it that turns
them ' unerringly' to the water? Smell? We are sorry that Newman's
notes are not more complete at this point. He tells us, however, that
the present study is only a preliminary one. We hope that when he
continues these investigations he will, if possible, introduce some con-
trol experiments looking to the analysis of the factors at work in chelo-
nian orientation.

On the whole, Newman has done his difficult and trying task with
a great deal of thought and care. This investigation gives us a mass
of useful data on the habits and instincts of the most important forms
of tortoises.   J. B. W.

*The Establishment of an Association Involving Color-Discrimina-
tion in the Creek Chub, Semotilus atromaculatus.* MARGARET
F. WASHBURN and I. MADISON BENTLEY. Jr. of Compar.
Neurol. and Psychol., 1906, XVI., 113–125.

The work reported in this article extended from July 31 to August
18, presumably of last year. The subject used was a female of the
common species of creek chub (*Semotilus atromaculatus*). Through-
out the experiment the fish was kept in a circular glass tank 50 cm.
in diameter and 45 cm. deep. The apparatus for feeding ' consisted
of two like pairs of dissecting forceps which were faced on the outer
surfaces with four-cornered strips of wood 5×5 mm. across and 70
mm. long.' These strips were fastened to the forceps by means of
small rubber bands, and projected about 5 to 10 mm. beyond the me-
tallic points.

The method adopted was designed ' to test color-discrimination by
establishing, if possible, an association between a certain color and
food.' The general method is worked out under two sub-methods :

I. *The Method of Inhibition.* — The colors of the strips in this
test were dark red on one of the pairs of forceps and a brighter green

on the other.   A small live grasshopper was held in the forceps.   Only one pair of forceps was used at a time.   When the fish was in a certain position at the bottom of the tank the grasshopper was thrust under the surface of the water.   " *The fish was allowed to take the food from the red forceps; but when it snapped at the green pair, the food was quickly withdrawn.*"   As a result of this method the authors tell us that " neither the times nor the observed behavior of the fish indicated any constant difference in the response to the two colors used."   It was decided that even though the fish associated the red color with success in obtaining food, it was unable to inhibit the reaction to the green stimulus.

II. *The Method of Choice.*— After about the fourth day (?), one hundred and thirty-one trials having been made, the above method was modified.   " *Both pairs of forceps were presented at the same time, the red baited and the green empty.*"   The tank was divided into two compartments by a thin wooden partition and an opening was left at each end of the partition, allowing the fish to pass freely from one side to the other.   " With the subject in compartment *A*, the forceps were suspended side by side in the middle of compartment *B* and about two inches from the partition.   They were held in place by being slipped vertically into narrow grooves sawed in a horizontal strip which ran across the tank just above the surface of the water, parallel with, and attached to the upper edge of the partition.   After the forceps had been set into position one of the gates, right or left, was opened and the fish allowed to swim to compartment *B* and to secure the food from the forceps."   To avoid errors of space the two pairs of forceps were constantly interchanged in position and the two gates so used that the fish was allowed to enter on the side of the bait only half of the time.

A few tests were made each day, under as nearly uniform conditions as possible, apparently, first, with the red forceps baited with mealworms, then from one to four tests were made with neither baited. This latter part of the test undoubtedly operated slightly against the formation of the association of food with red.   If the experimenters had used more subjects they could have avoided this error, and for other reasons the results would have been more satisfactory.   Due precaution seems to have been taken against the influence of the smell and the sight of the food.   From the fish's behavior the sight of the *food* seems to have had but little effect on the reactions and the smell probably had no effect.   The red strips were varied in brightness to make sure that the fish was not discriminating on that basis.   Some tests were made with blue instead of green strips.   The results are interesting.   Only the totals can be given here.

Total number of tests made from July 31 to August 9.............. 226
Red (with bait) chosen first... 169
Green [1] 13
Number of tests made with no bait 44
Red chosen 42
Green chosen... 2

From August 10 to 12 blue was substituted for the green. Thirty tests were made with the red baited and ten without. The red was chosen every time.

Now, after the association had been formed between the food and the red color the experiment was reversed, the bait being held in the green forceps. Every precaution taken above was, seemingly, observed in this test. The results follow:

Total number of tests (Aug. 13–18) 69
Red chosen first... 18
Green (now with bait) chosen first... 35

In sixteen tests with neither forceps baited, red and green were each chosen half of the times.

The experimenters conclude that the ' *Semotilus atromaculatus* distinguishes red from green and from blue pigments, the discrimination being independent of the relative brightness of the colors'; that such 'successes' as the getting of food have powerful enough consequences to *guide*, but not to *inhibit* 'an animal in the performance of an instinctive action.' The positive part of this conclusion seems to have good grounds, but the negative is certainly founded on too meagre data.

It is to be regretted that more subjects were not used and that the experiment was not carried on further. While the point at issue seems to have been pretty well established, so far as this subject is concerned, there are many other interesting problems that arise in the reader's mind. By testing many subjects as carefully as this one was tested, and by certain minor improvements in method, this experiment could be carried into a profitable examination of the formation and persistency of the 'associative memory' of the fish. The results would be fruitful to comparative psychology. It would seemingly be possible to exhibit the bait, if necessary, with *both* colors and thus absolutely to eliminate the possibility that the fish was reacting to the sight of the bait and not to the color. In one case the bait could be inside of a thin test tube. In a very careful test it would be worth while to regulate the light in some way so that it would be practically

[1] Most of the mistakes, *i. e.*, choosing the green, were made at the beginning of the tests.

uniform in intensity when the bait is viewed at different angles by the subject.

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Wasps Social and Solitary. GEORGE W. and ELIZABETH G. PECK- HAM. Boston and New York, Houghton, Mifflin and Co., 1905. Pp. 306 + xiv.

A part of the material here presented was published several years ago by the Wisconsin Biological Survey under the title 'Instincts and Habits of the Solitary Wasps.' The present book contains some new material, but in the main it represents a revision of the former work. In its present form the book is a good deal more accessible to the general reader than the former volume. To anyone familiar with the careful and painstaking work of the Peckhams, with their sometimes quaint and always charming style of presentation, the present volume needs no recommendation. Would that all so-called 'nature students' might take a much-needed lesson from the Peckhams and learn how to combine accuracy of observation with a clear, literary style of presentation!

The thirteen chapters in the book are devoted to the following subjects: I., Communal Life; II., Ammophila and her Caterpillars; III., The Great Golden Digger; IV., Several Little Wasps; V., Crabo; VI., An Island Settlement; VII., The Burrowers; VIII., The Wood-Borers; IX., The Spider Hunters; X., The Enemies of the Grasshopper; XI., Workers in Clay; XII., Sense of Direction; XIII., Instinct and Intelligence. Since it is impossible to review the book as a whole in any adequate way, we shall confine the critical part of our review to the chapter entitled the 'Sense of Direction.'

The Peckhams do not believe that the wasps have any innate sense of direction. They took definite steps to prove this position. Fifty-five yellow jackets were captured at their nest one morning. One observer carried the wasps out on the lake near an island to the distance of one eighth of a mile. The other remained near the nest to note the return of the wasps. The nest was closed the night before. On the morning in which these observations were made the 55 wasps were allowed to fly into the wasp cage, then the nest was again closed. This made it possible to observe the number of wasps who returned from the place of liberation. Twenty wasps were first set free. These all without exception flew toward the island and away from the nest. The remainder of the wasps were then carried farther away from the

island (one eighth of a mile) and liberated. They seemed much confused, some returned to the boat, alighted, but finally flew away ; others circled higher and higher, finally flying away in many different directions. Of the fifty-five wasps liberated thirty-nine returned in less than an hour. On account of the confusion on the part of the wasps when they were first liberated, and by reason of the fact that many of the wasps at first took a direction opposite to that of the nest, the authors conclude that the above species at least possess no innate sense of direction, but that they are guided back to the nest by visual landmarks and the ' memory ' of locality.

These experiments are not very convincing, in view of the fact that capturing the wasps entails the stirring up of the very powerful emotion of fear in them. Any one who is ready to ascribe a special sense of direction to the wasp might easily say that the fear had disturbed the normal functioning of the mechanism used in distant orientation. If this were true we should expect the insects to scatter in all directions. Several other experiments of the same general character were tried by the Peckhams, all leading to similar results.

An entirely different experimental procedure was adopted in testing the method of orientation of the solitary wasps. Many species of solitary wasps were observed in the act of making nests in *new localities*. It was found that during the process of nest building a solitary wasp will make many short flights, zigzagging about in all directions—spying out the neighborhood according to the Peckhams. Not until every nook and cranny in the vicinity has been explored will the wasp venture for any long distance after the indispensable caterpillar. According to our authors, many of the solitary wasps have great difficulty in finding their way back to the nest. The Peckhams had to be extremely careful not to disturb the details of the locality of the nest. Even the breaking off of a leaf which covered the nest caused a wasp of the species *Aporus fasciatus* to lose her way entirely. The nest was found immediately when the leaf was returned. These results are in agreement with those obtained by Loeb (*Physiology of the Brain*, p. 226).

It is interesting to compare these results with those obtained by Bethe in experimenting upon orientation in bees. Bethe was able to alter the face of the entire neighborhood of the bee-gum and to mask it in every conceivable way, but the bees returned to the gum without being confused in the slightest degree. The slightest displacement of the entrance-exit opening to the gum, however, — such as that produced by rotating the gum for 45° or more on its vertical axis — caused

the greatest confusion in the behavior of the bees.   They would return and congregate on the side of the gum where they had customarily found the entrance — only by degrees was the new location of the opening found and made use of.   One other interesting experiment of the same type was made by Bethe: Early one morning after the bees had practically all left a gum which faced the east, he drew it straight back for a distance of two meters.   The bees on returning collected en masse in the air around the old position of the gum, failing utterly for a long time to see the gum which was continually in plain view.   Vision would thus seem to play little part even in the adjustment of the bee to objects near at hand.

As far as concerns the giving of any crucial positive evidence upon the problem of *distant orientation*, this work of the Peckhams leaves us practically where we were before.   It does give us an accurate account, however, of the behavior of the solitary wasps in leaving the nest and in returning to it.   From the Peckhams all we may unequivocally conclude is that the solitary wasps make their proximate orientation largely by the aid of vision.   From Bethe's work on the bees we think it safe to say that the kinæsthetic impressions play a large part in orientation to objects close at hand.[1]

The book of the Peckhams is valuable as a whole because it gives us an accurate description of the types of behavior of many different genera and species of wasps.   J. B. W.

Contribution à l'étude du problème de la reconnaissance chez les Fourmis. H. PIÉRON.   Extrait des Comptes rendus du 6ᵐᵉ Congrès intern. de Zoologie, Session de Berne, 1904, 482–490.

The paper sets forth the results obtained by the above investigator from certain experiments designed to verify the rôle played by smell in recognition among ants.   Three species of ants were experimented upon, the *Aphænogaster barbara nigra*, the *Formica cinerea* and the *Camponotus pubescens*.   The first-named species is very deliberate of movement, of various sizes, almost blind, and of a warlike nature.   The last two are very agile, very keen-sighted, and extremely peaceable, preferring flight to a struggle.

In his experiments he used juices made by crushing some of the ants, filtering the resulting liquid and diluting it with pure water.   The experiments were repeated many times, neuters being used in every case.   Two series of experiments were carried on, one upon the ground, the other in a closed flask.

[1] Bethe, however, does not draw this conclusion.

The following results were obtained:

In the case of the *Aphænogaster barbara nigra*, when a neuter of one nest (*a*) was dipped in the juice of another nest (*b*) and was then placed among the neuters of (*b*), it was not attacked until the odor had evaporated. On the other hand, when it met a single neuter of (*b*) after being so dipped, it made an attack which was not reciprocated. Inversely, when a neuter of (*a*) was dipped in the juice of (*b*) and was then placed among the neuters of its own nest, it was instantly attacked. These attacks ceased after a time, presumably after the odor had evaporated. The results were practically the same when the ants were placed in a closed flask, although the abnormal conditions made the results less clean-cut.

In the cases of *Formica cinerea* and *Camponotus pubescens* no very definite results were obtained, either on the ground or in a flask, because these ants sought safety in flight.

A final series of experiments was carried on with the *Aphænogaster barbara nigra* and the *Formica cinerea*. The results obtained from the experiments conducted on the ground were poor, because the *Formica cinerea* avoided a struggle by fleeing before the *Aphænogaster barbara nigra* could make an attack. In a closed flask, an *Aphænogaster barbara nigra*, whether dipped or undipped in the juice of the *Formica cinerea*, was not attacked by the latter, but *Formica cinerea* was attacked by *Aphænogaster* whenever the latter could get near enough. Inversely, in the majority of cases, *Formica cinerea* dipped in *Aphænogaster* juice was not attacked by *Aphænogaster* until the odor had evaporated. *Aphænogaster* dipped in *Formica cinerea* juice was attacked by its own nest-mates. Nothing definite was obtained from the inverse case.

These meager results would probably not have been published if the author had read the classical and detailed work of Miss Adele M. Fielde on the Power of Recognition among Ants. The only excuse the reviewer offers for noticing the article is that it deals with species of ants different from those used in Miss Fielde's investigation.

<div align="right">MARY ICKES WATSON.</div>

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The Reactions of Ranatra to Light. S. J. HOLMES. Jour. Comp. Neur. & Psych., 1905, XV., 305–349.

The subject of study is phototaxis in the *Ranatra fusca*. The general purpose is to determine whether these phototactic responses as exhibited under the experimental conditions are to be explained wholly in reflex terms, or whether they can be modified by past experience

and may be partly explicable in terms of some 'pleasure-pain conception. The responses studied are certain swaying movements of different parts of the body, certain bodily postures and movements of the organism as a whole in relation to the light. The modifying conditions employed were the effects of contact, temperature, cerebral hemisection, the covering of various parts of the eyes, etc. *Ranatra* exhibits both negative and positive responses, the latter being the more normal or usual type. The type of response is experimentally controllable, the negative reaction being induced by contact, darkness, and diminished temperature. These negative responses are associated with a condition of sluggishness, or lowered phototonus, and are interpreted as due to changed conditions of the nervous system. Positive reactions may be persisted in till fatal results occur. Several experiments prove quite conclusively that past experiences may be operative in partly determining present conduct. The author is of the opinion that the method of trial and error plays only a subordinate rôle in making adjustments to novel situations.

The author claims, or rather perhaps admits, that all the responses are to be considered partly, and a great many entirely, as mechanical and reflexive in nature, and in fact the evidence abundantly confirms this position. Furthermore, he believes that certain of the reactions are partly determined or influenced by considerations of prospective pleasure or pain. The fact that positive phototaxis may lead to disastrous results is admitted to be a grave and serious objection to the view, but yet the author maintains quite fairly, I think, that this conduct under the unusual experimental conditions does not *necessarily* constitute an insuperable objection. The possibility of such modifying central conditions as it is necessary to assume is supported by the facts that light impulses must travel through the principal nervous centers, and that central influences are evidenced in the cases of learning and the change from negative to positive reactions. The positive evidence adduced in support of the contention is drawn from the experiments in which various parts of the eyes are covered without interfering markedly with the accuracy of the movements towards the light. These movements are difficult to explain by a reflex theory, are hardly the result of a progressive approximation by the trial and error method, but give the impression of an apparent felicity of effort. The author states the conclusion in a very conservative and tentative fashion, though it seems apparent by reading between the lines that he believes much more strongly in the truth of his conclusion than his words indicate, or for that matter than his facts

conclusively warrant. Probably this is a characteristic attitude of a large number of careful experimenters on animal behavior. A long course of observation on some organism leads to a pretty definite private conviction as to the existence of some degree of an intelligent type of behavior when the very phenomena inducing the belief are so vague, intangible and merely suggestive in character as to elude not only their experimental isolation, but even verbal description.

HARVEY CARR.

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The Selection of Random Movements as a Factor in Phototaxis. S. J. HOLMES. J. of Comp. Neurol. and Psychol., 1905, XV., 98–112.

The results of the experiments reported in this paper show that the act of orientation to light in the earth worm (similar experiments were made upon the leeches, the larvæ of house flies, blow flies and other insects) does not take place in accordance with the theory of ' forced orientation.' As is well known, the earth worm is negatively phototactic. " As the worm crawls it frequently moves the head from side to side as if feeling its own way along. If a strong light is held in front of the worm it at first responds by a vigorous contraction of the anterior part of the body ; it then swings the head from side to side, or it draws it back and forth several times, and extends again. If in so doing it encounters a strong stimulus from the light a second time, it draws back and tries once more. If it turns away from the light and then extends the head, it may follow this up by the regular movements of locomotion. As the worm extends the head in crawling it moves it about from side to side, and if it happens to turn it towards the light it usually withdraws it and bends in a different direction. If it bends away from the light and extends, movements of locomotion follow which bring the animal farther away from the source of stimulus."

In other words, the light induces a general state of activity leading to random movements.

The first movement induced by the light may be either towards it or away from it. If towards the light it is checked, the animal draws back, and movement, usually away from the light, then follows. Since this movement does not lead to further stimulation, it is prolonged farther. The final result of these random movements will thus eventually bring the less sensitive posterior end of the animal into the direction of the rays of light.

Holmes used the beam of the projection lantern (passed through an alum cell) for the stimulating light in these experiments. His method was essentially as follows: The worm was allowed to crawl on a wet board; when crawling in a straight line it was quickly lowered into the beam of a projection lantern in such a way that its body would lie at right angles to the rays.

According to Holmes the light reactions in the earth worm (and in the other forms mentioned above) are really a resultant of two motor responses: "First, the activities of locomotion which are set up by the stimulus of the light, and second, the act of jerking back and bending the body from side to side in response to a strong stimulus in front. Here are two instincts or reflexes, however we may be pleased to call them, which are in a measure antagonistic in that the first is frequently overcome by the second. The direction of the external stimulus determines which of these two instinctive tendencies predominates."

The author does not wish to put himself upon record as regards the problems whether conscious selection is present in these organisms, or whether any association is established between stimulus and reaction.

Reactions to Light and Mechanical Stimuli in the Earthworm, Perichœta bermudensis (Beddard). E. H. HARPER. Biol. Bulletin, 1905, X., 17–35.

Harper shows, in the present paper, that while Holmes is right in concluding that the act of orientation in the earthworm brought about by the influence of a beam of light of the intensity of the projection lantern is accomplished by the following up of random movements, he is not right in supposing that all acts of orientation to light in this organism take place in accordance with this method. On the contrary, if the light stimulus is made more intense the orientation is direct. If a worm crawling in a straight line is lowered suddenly into a beam of direct sunlight, the anterior end is turned away from the light, and by a series of turns the worm gets into an oriented position and crawls directly away from the light. "Usually the result is produced without a false movement." If the paper or board upon which the worm is crawling is turned as the worm turns, in such a way that the rays of light are kept at right angles to the path of the worm, the worm may thus be made to travel continuously in a circle without trial movements.

The earthworm is thus oriented directly by light, but owing to the low degree of sensitiveness its movements are uncertain except in light

of great intensity. Harper agrees with Holmes that the first effect of light is to produce general restlessness inducing locomotion. In light not strong enough to produce direct orientation, the worm projects its anterior end in any direction. If towards the light, the worm after stretching out its anterior end will again retract it as if stimulated. If the worm is checked only after making an extension movement towards the light the conclusion would seem to be, says Harper, that the anterior end is more sensitive when extended than when in the contracted condition. He then shows by further experiments that this is the case. If a worm in the contracted state is suddenly stimulated by a strong light, the stimulus leads only gradually to movement. If, on the contrary, one shades the light from a worm crawling on moist paper, the worm will make a sudden response by jerking back its head the moment it is protracted into the light.

Harper thinks that the difference between the sensitivity of the earthworm to light in the contracted state and in the extended state will account for the random movements observed by Holmes. The worm begins these random movements when in the contracted state, and consequently when its sensitivity to light is least. " The nature of its locomotion and of the sensitive elements in its skin necessitate the alternation of states of low and high sensitiveness. The random movements of an earthworm under light stimulation are consequently of an entirely special character, due to causes inherent in its structure."

The author then shows that the anatomical basis for this alternation of high and low degrees of sensitivity consists in the fact that the light cells of a worm in the contracted state (the light cells are to be found in the basement membrane) are shut off from the source of light by means of the thickening of the epithelial layer and to the in-rolling of the most sensitive regions, viz., of the anterior end in which the light cells are most numerous.

Harper next tested the sensitivity of the earthworm to light under conditions which approximate the normal burrow life of that animal. In the burrow the movements are longitudinal. He found that the axial movements initiated by the anterior and posterior ends are more definitely controlled by the stimulation of light and by a weaker stimulus than are the lateral movements. Lateral movements tend more to be random and are directed only by stronger stimuli because the organization of the worm is chiefly adapted to burrow life and not to an open air life.

Dr. Harper's paper is an excellent contribution on the experimental side. In his general conclusions, however, some general statements

are made which we cannot agree to if we have interpreted them correctly. After concluding that the reactions of the earthworm are far removed from the sort of 'trial and error method' of the Infusoria, as analyzed by Jennings, and that they do take place by a 'direct reflex,' he tells us: "Methods of trial and error in reaction to light and other ordinary stimuli have clearly been supplanted by more definite responses in all but the protozoa and certain other low types of animal life. * * * For the trial and error method is clearly supplanted in the ascending scale of animal life by reactions of definite nature, in the case of responses to the ordinary stimuli."

Certainly we were of the opinion, at least until the advent of Jennings' work on the protozoa, that the method of trial and error is exemplified *only in the ascending scale of animal life*. Our chief reason for welcoming the work of Jennings is that if his observations are true that protozoa adopt the trial and error method, we are enabled to unify our conception of animal behavior and to bring all types of behavior under the one rubric.

<div align="right">J. B. W.</div>

Some Points Regarding the Behavior of Metridium. LULU F. ALLABACH. Biol. Bulletin, 1905, X., No. 1.

Certain reactions of *Aiptasia* to food were found by Jennings to be due to a state of hunger. What these reactions were this paper does not say. It tests the part hunger plays in reactions of *Metridium*. It was found that as hunger decreased the reactions to food became slower. After a time the outer tentacles refuse filter paper (soaked in meat juice) and later meat. Next the inner tentacles refuse first the filter paper and then the meat. Lastly if filter paper is placed on the mouth it is not ingested. The mouth never refuses meat, but 'from lack of assistance from muscular contraction' it becomes impossible for the cilia to draw the food inward. Whether this lack of muscular assistance is due to fatigue or to satiety we are not told. That when the tentacles cease to react it is not due to fatigue is proved: for, if *Metridium* is satiated, having been fed from one side of the body only, the tentacles on the other side which have not been fatigued refuse food. Moreover, they continue to refuse food for several hours, and fatigue lasts but a few minutes.

Fatigue does occur, however, and explains the reactions described by Nagel in an earlier paper. Long before *Metridium* is satiated the outer tentacles of any one spot refuse, after a short time, first the filter paper and then the meat; but the tentacles on other parts of the

body will still accept food for the time being. Miss Allabach made certain experiments which seem to disprove Nagel's idea that these reactions are due to judgment on the part of *Metridium*. The food was removed from the œsophagus. The tentacles behaved as before. The food had not been digested and *Metridium*, therefore, could not judge by experience that the food was not good. The conclusion is that the tentacles become fatigued.

The problem is a significant one. The method, so far as it goes, is good. Both suffer because Miss Allabach's report is inadequate and not well organized.

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REPORTS.

MEETING OF EXPERIMENTAL PSYCHOLOGISTS.

On April 18 and 19 the third annual meeting of Experimental Psychologists was held in the Yale laboratory. No formal program was made up before the meeting and no papers were read. Informal reports were given by representatives of the different laboratories, of investigations under way, and of those recently completed but not yet published. Following each report there was much free discussion. On Wednesday evening the members dined at the Graduate Club and spent the evening in an informal smoker.

On Wednesday morning Professor Titchener reported for Cornell, and characterized the work of that laboratory as being in general an experimental reëxamination of much of the matter which has been current in psychology. He specifically mentioned three lines of investigation: first, a more exact introspective analysis of the feelings; second, a like description of the commoner organic sensations designated in every-day speech by such expressions as 'a lump in the throat'; and third, a series of tests of mental ability. After a discussion the morning session was concluded by the reading of a report from the Chicago laboratory in the absence of its authors, Drs. Carr and Allen. This paper reported a case of voluntary localization of objects in depth through change of accommodation while convergence remained the same, and of voluntarily holding an object at a given localization while convergence varied. The discussion of this report extended into the afternoon and brought out a report by Professor Heinrich, of Krakau, on similar work done by him in Poland. His

general conclusion, in opposition to the conclusion of Drs. Carr and Allen, was that changes in accommodation affect localization only indirectly through the resulting changes in the retinal image.

The Wednesday afternoon session was occupied with reports from Professors Sanford and Dodge and a short report from Mr. Loomis of the Yale laboratory. Professor Sanford reported investigations in learning as the chief subject of research at present going on in the Clark laboratory. He mentioned particularly the learning process which is involved in the use of the typewriter, and in playing chess, with an accompanying introspective analysis of this process. He also mentioned studies on the favorite number, as it appears in guessing contests, and of the reasoning processes as revealed, for example, in the solution of problems of division. Professor Dodge reported an investigation on the influence of the pre-exposure and post-exposure fields on the length of time necessary for the recognition of visual impressions. He found that the length of time was markedly different for differences in the complexity and illumination of the pre-exposure and post-exposure fields, and concluded, in general, that the change is due to a peripheral process. Mr. Loomis reported graphic records of the movements made by various subjects in lifting weights subject to the familiar illusion which appears when the weights are of different bulk, but of like intensity.

On Thursday morning Professor Woodworth reported for Columbia. He referred briefly to investigations of a statistical nature, to an investigation on reading, to others on practice, on the relation of the position of the body to mental activity, on reaction times, on color mixing and on the capacity for learning as shown in the behavior of paramecium. He also reported more fully an investigation of the cue to voluntary movements. This investigation was carried on by means of introspection during various voluntary acts. The results pointed to a mental state, having as its chief element something different from the sensational or motor content and corresponding on the physiological side to processes in the association areas. Dr. Baird reported for Johns Hopkins. He described an investigation on the reverse of the weight illusion, in which the size remained constant and the weight varied, and a study of the perception of direction during movements of varying amplitude on the skin. He reported more fully an investigation of the relation of the pigmentation of the macula to the perception of color stimulating that region of the retina. Messrs. Cameron and Freeman of the Yale laboratory occupied the remainder of the morning session in reports of investigations on tonal production and distraction, and on writing.

During the afternoon Professors Holt and Pierce and Dr. Porter made reports, and the equipment of the Yale laboratory was inspected. Professor Pierce reported an investigation on stereoscopic fusion. He found that in the large majority of the cases which he investigated stereoscopic fusion did not take place and there was either false or equivocal interpretation. Dr. Porter reported a continuation of his investigations on the variability of instinct in nest-building spiders, and an investigation on reasoning in abnormal subjects. Professor Holt's report dealt with the thresholds of depth recognition through changes in convergence, with color vision in the immediate neighborhood of the blind spot and with the effect of variations of pitch on the localization of sounds. During the session the exercises which make up the experimental course at Yale were described and the apparatus used in this course was demonstrated. Professor Dodge also demonstrated the exposure apparatus with which he made the investigation on visual recognition above mentioned. It was so arranged that successive fields could be exposed by transfer of light from one to the other by mirrors, without motion of the field.

The following members were present and participated in the discussions, though they presented no reports: Dr. Yerkes, Professors Thorndike and Davis.

The invitation of Professor Witmer to hold the next meeting at the University of Pennsylvania was unanimously accepted.

FRANK N. FREEMAN.

YALE UNIVERSITY.

BOOKS RECEIVED FROM APRIL 5 TO MAY 5.

Le sentiment et la pensée. A. GODFERNAUX. 2e éd. revue. Paris, Alcan, 1906. Pp. 205. Fr. 2.50.

Idées générales de Psychologie. G. H. LUQUET. Paris, Alcan, 1906. Pp. 295. Fr. 5.

Congress of Arts and Science. Vol. II. Politics, Law and Religion. Vol. III. Language, Literature and Art. Ed. by H. J. ROGERS. Boston, Houghton, Mifflin & Co., 1906. Pp. 661 and 680. $2.50 each.

Enigmas of Psychical Research. J. H. HYSLOP. Boston, Turner & Co., 1906. Pp. xi + 427.

On Life after Death. G. T. FECHNER. Trans. by H. WERNECKE. New ed. Chicago, Open Court Co., 1906. Pp. 135.

The Recitation. S. HAMILTON. Lippincott's Educ. Series, ed. by
M. G. BRUMBAUGH. Philadelphia, Lippincott, 1906. Pp. xi
+ 369.

*Greek Theories of Elementary Cognition, from Alcmæon to
Aristotle.* JOHN I. BEARE. Oxford, Clarendon Press; New
York, Frowde, 1906. Pp. viii + 354. 12s. 6d. net.

NOTES AND NEWS.

DR. J. W. BAIRD, of the Johns Hopkins University has accepted
an appointment as instructor in experimental psychology in the
University of Illinois.

FRANK THILLY, Stuart professor of psychology at Princeton Uni-
versity, has accepted a call to Cornell University, as professor of phi-
losophy in the Sage School of Philosophy.

Science announces that Dr. Kate Gordon, associate professor of
psychology in Mt. Holyoke College, has been appointed instructor in
educational psychology in Teachers College, Columbia University.

THE Psychological Laboratory of Bryn Mawr College will open
next Fall in new, commodious and well equipped quarters in a wing of
the Library Building. It will be as heretofore under the direction of
Professor Leuba.

PROFESSOR JOHN DEWEY, of Columbia University, has been ap-
pointed Lecturer in Greek Philosophy at the Johns Hopkins University
for the year 1906-7. He will give a course of ten lectures on ' Prob-
lems of Greek Philosophy,' beginning after Thanksgiving.

FOLLOWING the suggestion of one of our correspondents, who
finds some confusion among our different publications arising from
their identical color, we are adopting a new cover for the MONOGRAPHS
and also one for the INDEX. The BULLETIN will continue to appear
in white and the REVIEW in blue.—ED.